LETTERS TO THE EDITOR.

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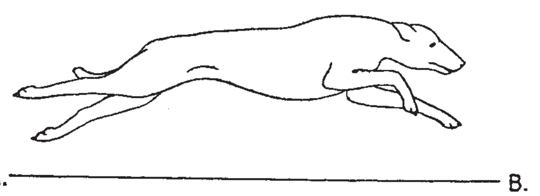
The Gallop of the Horse and the Dog.

IN a note in NATURE of October 28 (p. 526) it is stated that Mr. Francis Ram, in a recent book, says I am in error (in an article lately published by me) in regard to the position of the legs and feet in a running dog.

I have not seen Mr. Ram's book, but I should be glad if you will print the enclosed outline figure of a running dog taken from a series of instantaneous photographs of a running dog by Mr. Edward Muybridge.

The horizontal line AB gives the actual level of the ground below the dog. The figure is one drawn for a book which I have in preparation, and I think has considerable value, since it serves to establish my suggestion that the Mycenæans (who were the originators of the pose of the galloping horse, which was never used by Greeks, Egyptians, Assyrians, Romans, or Europeans, but travelled, as Salomon Reinach has shown, across Tartary to China and Japan, and came from Japan to England at the end of the eighteenth century) did not *invent* the well-known conventional pose, but *observed* it in the dog, and very reasonably, but incorrectly, *applied* it to representations of the horse and other animals which do not really assume that pose. The pose in question satisfies the



artist's judgment even when applied to the horse, because the outstretched position of the hind legs, with upturned hoofs and the forward-reaching position of the fore-legs, do *succeed* one another in the galloping horse so rapidly as to cause, not a continuity of the retinal impressions, but a continuity of the more slowly formed mental appreciations of the positions of the legs.

It is an important fact that the late Prof. Marey, of Paris, did not succeed in photographing the dog with all the feet "off" the ground and the legs in the position shown in Muybridge's photographs, and consequently archæologists have supposed that the Mycenæans imagined the pose as an artistic expression of rapid galloping. It seems to me, on the contrary, certain that they constantly saw and admired this pose in their hunting dogs.

E. RAY LANKESTER.

29 Thurloe Place, South Kensington, October 29.

The Refractivity of Radium Emanation.

WE have read with special interest the communication from Lord Rayleigh in NATURE, October 28 (p. 519), on the determination of the refractivity of gases available only in minute quantity, because we ourselves have been working towards the same end at intervals during the last two years. Our object in view was also the same, viz. the determination of the refractive index of radium emanation; not only for the intrinsic interest of a knowledge of the refractivity in question, but also because of the great probability of the emanation being one of the series of non-valent elements, and the determination would therefore enable us to extend the series of simple integers which has been found by one of us to connect together the refractivities of the other elements in the series.

The extremely minute quantity of emanation available—not more, after undergoing the ordeal of purification, than about one-tenth of a cubic millimetre measured at atmospheric pressure—made it quite clear that the refractometer to be employed must be on a minute scale, and the form which it seemed to us would probably lead to the most accurate results in the circumstances was one on the principle of a Fabry and Perot *étalon*, partly on account of the sharpness of the bands thus obtainable and partly because it is the *double* thickness which constitutes the path difference between successive interfering beams, and consequently the gas contained is utilised twice.

A capillary tube of glass (or fused silica) was sealed at one end, and a transverse hole was drilled passing through the extreme end of the bore. Two parallel faces perpendicular to the axis of the hole were then ground on the tube, and parallel plates of glass (or silica), silvered (or platinised) on the inside, were then cemented on the faces with Coate's cement. For this apparatus we had recourse, as usual, to the excellent workmanship of Messrs. Hilger. The result was a tiny interferometer vessel, 2·271 millimetres long and 0·71 mm. diameter, into which we could compress the emanation column through the capillary tube by means of a mercury column in the usual way. When this interferometer was set up in the path of the green beam separated spectroscopically from the light given by a Bastian mercury lamp, and the light passing through was examined through an astronomical eye-piece—the lens system throughout being chosen so as to give best illumination—the interference bands which were obtained were all that could be desired, it being easily possible—when the silvering was of the best thickness—to measure micrometrically to the hundredth part of a band.

The method of a determination, in general outline, consisted in alternately increasing and decreasing the pressure of the contained gas from and back to a practically zero value and observing the number of interference bands which passed over the cross-wire of the micrometer. In order to determine the efficiency of the arrangement, observations were made for the refractivity of air, with the result that we think we are justified in claiming that an accuracy to within about 2 per cent. could be relied upon, so far as the optical part of the experiments is concerned.

The real difficulties begin, however, when we deal with the emanation itself. The rapid generation of impurities, originating in part in the action of the emanation upon the resinous cement employed for fixing the parallel plates, together with the lack of a knowledge of what these impurities are, made it impossible to calculate the index of the emanation from the experimental results, although it was perfectly easy to measure the refractivity of the mixture of gases existing at any time. The only datum known in regard to the composition of the mixture was the approximate percentage of emanation present, this being found by measuring the γ radiation from it. The direction in which the refractivity lies may, however, be inferred with probability from the following observations. Starting with emanation given off from a solution and containing a very large amount of impurity, this was purified, first, by explosion, drying, and absorption of CO_2, and afterwards by freezing in liquid air and pumping off the volatile impurities according to well-known methods. Testing the refractivity from time to time, its value—at first of the order of that of air—did not sensibly rise until the volume was about one cubic millimetre. Continued purification increased the refractivity, and the highest values obtained in our experiments were 0·000840 when the volume was 0·205 mm.^{3} (at atmospheric pressure) and 0·000916 when the volume was 0·128 mm.^{3} measured at atmospheric pressure. The quantity of emanation was approximately the same for both these measurements, and equalled the quantity in equilibrium with 0·178 gram of radium. Of course, if we could assume that the impurities were the same in kind on the two occasions it would be possible to estimate from these data the value for the pure substance, but the failure of this method on many occasions to give consistent results took away all belief in its applicability. For purposes of comparison we may state that the higher of the above values is about twenty-six times the value for helium, while the value for xenon—the highest for any known gaseous element—is twenty times, and for CO_2 thirteen times, the value of helium.

One source of difficulty so long as the available amount of emanation is so small is that the maximum pressure to which it can be raised in the apparatus is only a few (7 or 8) centimetres. The capillary correction thus becomes

exceedingly important, and Sir W. Ramsay has given reasons for believing that the capillary behaviour of mercury is quite abnormal in the presence of emanation. Another serious difficulty with which we had to contend was that, under the action of the emanation, the silver (or platinum) through which the light had to pass gradually became opaque. The consequence was that the apparatus had each time to be dismounted after a couple of days, the faces re-polished, re-silvered, and re-installed before a new experiment could be begun. This source of inconvenience would, of course, not be present in apparatus similar to the Young-Arago method employed by Lord Rayleigh or in a Jamin refractometer (which we think is the more satisfactory of the two), but we do not think that it would be possible to obtain an equal optical efficiency with these arrangements.

The amount of success attending these experiments—small though it may seem to be—justifies us in hoping that if the amount of available emanation were increased a few times only an approximate value of the refractivity would be ascertainable. Even at the present time this might be effected by means of a collaboration amongst all those in the United Kingdom who possess large quantities of radium.

In conclusion, we desire to express our thanks to Sir W. Ramsay for generously supplying us with the emanation with which these experiments were made.

ALFRED W. PORTER.
CLIVE CUTHBERTSON.
Physical Department, University College, London, November 1.

Atmospheric Cloudy Condensation.

IN NATURE of October 21 Sir Oliver Lodge, writing on the recent magnetic storm, seems to think that during these storms the sun is emitting electric projectiles which cause the magnetic disturbance, and that these projectiles will, at the same time, affect the rainfall by the influx of "cosmic nuclei." From this, I presume, Sir Oliver means that the electrons passing through our atmosphere will produce ions in the air, and that these ions will become nuclei of condensation, and in this manner may increase temporarily and locally the rainfall. Now, so far as is at present known, it does not seem probable that these electrified nuclei play any part in cloudy condensation. That they can become centres of condensation is not doubted, but before they can act in that way the atmosphere has to become very highly supersaturated.

These ions, therefore, cannot play any part in the condensation unless all the dust in the air be first removed. The question thus becomes, Is there such a thing as dust-free air in our atmosphere? So far as I am aware, no such condition has ever been observed. I have returns of observations made in many parts of the world by different observers, as well as by myself. Some of these tests were made while crossing the Atlantic Ocean, others on the Pacific Ocean. Many were made in this country and in different countries on the Continent. Some were made at sea-level, others up to an elevation of 13,000 feet, but none of these records shows anything like dustless air. Mr. Rankin, in his Ben Nevis report, says "any number less than 100 particles per cubic centimetre is phenomenally small." Mr. E. D. Fridlander, at an elevation of more than 13,000 feet on the Bieshorn, found 157 particles per c.c. In many hundreds of observations made by myself on the Rigi Kulm (6000 feet) nothing quite so low as 200 per c.c. was ever observed. The reports of the observations made on the oceans show the dusty air to be everywhere, and there does not seem to be much chance of ever finding dustless air, at least so low as cloud-level, as the air with least dust is not found in the descending currents of anticyclones, but in the cyclonic areas, where the air is well washed by the rains. It may be further stated, in connection with this subject, that there is no reason for supposing that an increase in the number of nuclei would have any effect on the rainfall, as in nature only a few of the nuclei do all the condensation, while the others remain inactive.

This letter may seem longer than the subject warrants, but my reason for entering so fully into the subject is that the idea is now very generally accepted that ions do form the nuclei of cloudy condensation in our atmosphere. So stereotyped has this theory become that there is not a scientific book recently published in which this subject is treated which does not give this view. Now, so far as our knowledge at present goes, there is no support for this theory, and those who advocate it will require to show that there is ever dustless air at cloud-level. I have elsewhere shown that, even supposing there was dust-free air, clouds would not likely be formed, but the supersaturation would be relieved by the direct formation of rain, as the condensation in the highly supersaturated air would take place on only a few nuclei, which would grow very rapidly to rain-drops.

It is not here contended that the passage of the electrons through our atmosphere will have no effect on the rainfall, only that it has not been shown that there are ever the conditions necessary for the ions so formed to act as nuclei of cloudy condensation. That the electrons may act in some way in determining the coalescence of cloudy particles to form rain-drops seems possible, but, so far as I am aware, even this has not been demonstrated.

Ardenlea, Falkirk. JOHN AITKEN.

Magnetic Storms.

J'AI lu avec le plus grand intérêt dans le No. 2083 de ce périodique la note importante de M. le docteur Chree sur la dernière grande perturbation magnétique du 25 septembre, 1909.

Comme je fais depuis 1882, j'ai cherché de la mettre en relation avec le passage de la grande tache solaire australe qui a été observée, dessinée et relevée à l'observatoire de Catane par l'assistant M. L. Taffara tous les jours depuis l'apparition au bord est le 18 septembre jusqu' à la disparition au bord ouest le 29 septembre, excepté les jours 19 et 22, où l'observation a été empêchée par les nuages.

De nos observations il resulte que la tache doit être passée par le méridien central le 23 septembre, environ à 5h. soir, temps moyen de Greenwich.

Dans la relation de M. Chree n'est pas donné le temps du maximum de la perturbation, parceque les oscillations des appareils magnétiques de l'observatoire de Kew étaient souvent plus amples de ce que pouvaient être enregistrées. En considérant le commencement et la fin des oscillations plus amples, on a les temps 11h. 43m. et 8h. 30m. et la moyenne 4h. 7m.

Si l'on fait la différence avec le temps du passage de la grande tache, c'est à dire sept. 25, 4h.–sept. 23, 5h. = 1j. 23h. = 47h., on a le retard de la perturbation magnétique sur le temps du passage de la tache à la moindre distance du centre du disque solaire, c'est à dire à la moindre distance de la droite qui unit le centre du soleil à la terre.

Ce retard est très peu différent de celui de 45½h. que j'ai trouvé en moyenne pour 8 coïncidences de passages de grandes taches avec le maximum de fortes perturbations magnétiques du premier semestre 1892; ce retard aussi n'est pas trop différent de celui de 42½h. que j'ai trouvé entre le temps moyen du commencement et de la fin de 19 grandes perturbations magnétiques et le temps des passages de grandes taches, ou de grands groupes de taches, d'après les relèvements de M. Maunder de l'Observatoire de Greenwich.

Il serait donc confirmé aussi pour cette grande perturbation magnétique un temps de 40 à 50 heures pour la propagation du soleil à la terre de l'influence ou action solaire sur le magnétisme terrestre; ce qui donnerait une vitesse de 900 à 1000 km. par seconde; c'est à dire une action plus que 300 fois moindre de celle de la lumière et des actions électromagnétiques. Mais cette vitesse pourrait bien être celle des particules émises par le soleil, selon les idées de M. Arrhenius.

Ainsi l'hypotèse corpusculaire de l'influence solaire acquiert toujours une plus grande probabilité.

L'assistant M. le Dr. Horn a fait à l'Observatoire de Catane presque à tous les jours du passage de la grande tache les photographies au spectrohéliographe, mais on n'a pas obtenu autour de la tache des très-grandes masses faculaires.

A. RICCÒ.
Catania, October 10.